

however, be objected that the doctrine which Mr. Darwin elsewhere inculcates,* and deems sufficient to account for the total suppression of rudimentary organs, viz., inheritance at earlier periods of life, is fatal to dysteleology as a prop to evolution—at least in the case of long-lived species. And so it would be, were not this principle of so shadowy an application that, while it is perfectly legitimate to point to it as a possible cause of total suppression in some cases, it would be simply absurd to argue that such must be its effect in all.

We next come to the Economy of Growth. Suppose an organ to become suddenly useless, this principle would at first cause its rapid reduction. In proportion, however, as its presence ceases to be injurious, the arresting process becomes slower and slower, until a point is reached at which it is presumably *nil*. That such a point of rest must somewhere be attained seems evident, if we consider that the smaller the diminishing organ becomes the less is it subject to the influence of the Economy of Growth. In other words, when the organ undergoing reduction becomes so minute relatively to the size of the animal (or, more correctly, to the available store of nutrition), that the supply of nourishment it requires is no longer perceived by the organism at large, it then remains permanently of that size. "The economy of growth will not account for the complete or almost complete obliteration of, for instance, a minute papilla of cellular tissue representing a pistil, or of a microscopically minute nodule of bone representing a tooth;"† and, the whole principle being one of relation, it is a question, for instance, whether the rudimentary digits of a horse consume a greater relative amount of nutrition than does the "minute papilla." Besides, without entering into details, I think there is very good reason to believe that the Economy of Growth is unable to reduce an organ which was originally large, to the same absolute size as it can an organ which was originally small. From all this it follows, that if the struggle for existence were in any case so keen as to afford Selection (*i.e.*, Economy) the opportunity of totally obliterating every rudimentary organ, it seems probable that the species itself would require to become extinct.

Turning now to the last of the causes propounded by Mr. Darwin, there can be no doubt that it is (theoretically) sufficient to procure total obliteration. Forasmuch, however, as we can never know in any given case whether or not the requisite conditions have been supplied,—*i.e.* impoverished nutriment for an enormous length of time,—this newly added cause affords no further justification for the old statement, that the theory of Natural Selection fails to account for all the facts of Dysteleology.

The perusal of the last-mentioned thoughtful conception has suggested to me the probable existence of another cause, having a more general application; but as it can never induce complete suppression, I shall reserve it for the subject of another communication.

Mr. Mivart supposes that organs may become suddenly aborted‡; but, apart from the weighty objections to this view,§ there is no case on record, so far as I am aware, of an organ thus becoming *totally* suppressed in any domestic species. A sport of this kind always leaves a rudiment, and it is upon the analogy of such sports alone that Mr. Mivart's argument is founded.

Having now enumerated all the causes ever proposed by evolutionists to account for the reduction of useless parts, it is evident that we should antecedently expect to find innumerable examples of such parts in the condition of rudiments.¶ Indeed the only difficulty is to account for that final disappearance of organs which must, by any theory of evolution, be postulated to have taken place. The solution is afforded by the exhaustive contemplations of Mr. Darwin, for, whether or not we believe in pangenesis, we cannot but deem it in the highest degree probable

upon until the point of such exhaustion is attained; but is it not open to question whether this point can never be reached at all? It must be remembered, too, that in the case of wild species the effects of disease are always associated with other reducing causes, so that here we may easily over-rate the share it has in the work; but in the case of domesticated species the effects of disease are much more isolated (in consequence of Economy of Growth, etc., being, to a great extent, in abeyance); and here we find that atrophy of disused parts, although at first very rapid, eventually does not proceed to nearly so great an extent as it does in the case of wild species. The question thus raised, however, is of no practical importance, since whether or not disease is the chief cause of atrophy in species, there is no doubt that atrophy accompanies disease.

* "Variation," vol. ii. p. 80.

† "Variation," vol. ii. p. 397.

‡ "Gene is of Species," 1st ed. p. 103.

§ See "Origin," pp. 201–204.

¶ It is unnecessary to consider the *collective* action of these causes, for a moment's reflection will now make it evident that none such exists below the point at which the Economy of Growth ceases to be felt.

that the influence of inheritance is not of unlimited duration. If so, we have at once an adequate cause for the eventual destruction, even in the embryo, of rudimentary parts; but, as it is a cause which would only act after an immense lapse of time, it would have no influence until the original specific type had undergone a considerable modification. Thus, the facts of dysteleology, far from "cutting two ways," afford the strongest confirmation of the natural selection theory; for, as time is thus shown the chief agent in the final destruction of rudiments, and as species are always undergoing change, on the one hand we have an explanation of the fact, that the greater the divergence of the specific type from its original the fewer rudiments do we find of organs characteristic of the latter, while on the other hand, the less such divergence the greater the number of such rudiments—a fact of which the necessary consequence is, that "with species in a state of nature, rudimentary organs are so extremely common that scarcely one can be named which is wholly free from a blemish of this nature." GEORGE J. ROMANES

The Action of the Heart

HAVING replied to Mr. Garrod's criticism of my "Locomotion of Animals" (NATURE, vol. ix. p. 281), I now wish to show that the explanation given by him of the diastole of the heart is not in accordance with fact.

In a recent number of NATURE (vol. ix. p. 282) I asked Mr. Garrod to explain "how the left ventricle of the heart opens after a vigorous contraction in which all the blood contained in the ventricular cavity is ejected and the ventricle converted into a solid muscular mass, if not by a spontaneous *elongation* of all its fibres." He replies:—"At first sight it might seem that the active dilatation of the heart during the diastole did depend on an inherent power in the muscular fibres of the ventricles to elongate, but the peculiarities of the coronary circulation are quite sufficient to explain the phenomenon without the introduction of so unnecessary a theory as that of Dr. Pettigrew. For in the heart when removed from the body, as in the living body during diastole, the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur" (NATURE, vol. ix. p. 301).

The explanation given by Mr. Garrod of the manner in which the ventricles of the heart open up during the diastole is eminently unsatisfactory. In fact it is no explanation at all. He informs us that the active dilatation of the ventricles is due "to peculiarities in the coronary circulation" . . . "for in the heart when removed from the body the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur."

The coronary vessels, as everyone is aware, simply supply the blood which nourishes the substance of the heart. There is no peculiarity whatever in the circulation of the blood through them. The blood flows through the coronary vessels in a more or less steady stream as through all the other vessels of the body. In other words the substance of the heart is full of blood during the closure or systole of the ventricles, as well as during the opening or diastole of the ventricles. The presence of the blood, therefore, within the coronary vessels can exert no influence in opening up or actively dilating the ventricles. This is proved by direct experiment. If the heart be cut out of the living body and the coronary vessels divided, the ventricles go on opening and closing with the utmost regularity for protracted periods. Here, however, the power which, according to Mr. Garrod, opens the ventricles is inoperative. The same thing happens when the heart is cut out of the body and the vessels laid open. If, however, the ventricles open and close when the coronary vessels are freely divided, and the blood which is said to distend or open up the ventricles is allowed to escape from the cut surfaces, it is quite clear that the blood pressure within the ventricular walls can exert no influence whatever in producing the diastole. If blood is not admitted into the coronary vessels, or if admitted it is allowed freely to escape, it cannot of course act as a distending medium. Allowing, however, for the sake of argument, that the flow of blood through the vessels of the ventricles occasioned the opening or dilatation of the ventricles, it is evident, for the same reason, that the presence of the blood within the ventricular walls, from the fact that the blood is nearly constant in quantity and virtually incompressible, would prevent the closing or contraction of the ventricles. Mr. Garrod, I opine, is

here on the horns of a dilemma. He evidently puts the cart before the horse. It is the movements of the heart which determine the movements of the blood, and not the converse.

The cardiac movements are due to a change of shape in the sarcous elements or ultimate particles of the muscular fibres of the heart, and in the adult organ can only be effected by a vital and alternate elongation and shortening of all the fibres composing the heart; the elongation occurring during the diastole and the shortening during the systole. Similar remarks are to be made of the voluntary muscles which, as stated in my work on "Animal Locomotion," are endowed with centrifugal and centripetal movements.

That the opening and closing of the ventricles of the heart are in no way connected with the passage of blood through the substance of the organ, is proved indirectly by the movements of the heart of the embryo. Here the heart opens and closes with time-regulated beat, while yet a mass of cells, and before it contains blood either in its cavities or in its substance. But that the presence of blood is not necessary to such movements is placed beyond doubt, for rhythmic movements occur in the vacuoles of certain plants, as e.g. the *Volvox globator*, *Gonium pectorale*, and *Chlamydomonas*, where no blood is present.

Lastly, if a frog be slightly curarised and its spinal cord destroyed, it is found, on exposing the heart, that the sinus venosus, vena cava inferior, the auricles and ventricles are quite destitute of blood, and yet the organ beats normally and with the utmost regularity. Mr. Garrod has consequently not yet succeeded in answering my query as to how the diastole of the left ventricle is produced. He has failed to show that it is not effected by the active elongation or centrifugal movements of all its fibres.

J. BELL PETTIGREW

Lakes with two Outfalls

HAVING observed the discussion lately carried on in your pages as to the existence of lakes with two outfalls, I think the following description of such a lake by Prof. Bell, of the Geological Survey of Canada, may be interesting to some of your readers. It occurs on the summit of the high Laurentian country between Lake Superior and Hudson's Bay:—

"In crossing the country from Lake Nipigon to the Albany River, we first followed the Ombabika River to its source, which is in Shoal Lake, three and a half miles long and one mile wide, lying at a distance of twenty-five miles north-east of the mouth of the river. This lake lies due north and south, and discharges both ways, the stream flowing northward towards the Albany, called the Powitik River, being nearly as large as the southern outlet. No portage occurs on the Ombabika for about nine miles before reaching Shoal Lake, nor for nearly five miles beyond its northern outlet; so that we passed the height of land with the greatest possible ease, having had about seventeen miles of uninterrupted canoe navigation, from the time we made the last portage, in going up the southern side, till we came to the first on going down on the northern. Shoal Lake has an elevation of scarcely 300 ft. over Lake Nipigon, or about 1,200 ft. above the sea."—"Report of Progress Geological Survey of Canada for 1871-72," p. 107.

GEORGE M. DAWSON

Montreal, Feb. 19

The Ink of the Cuttle-fish

WITH reference to the interesting account in NATURE, vol. ix. p. 332, of a gigantic Cephalopod captured in American waters, and of a still larger one, which attacked the boat belonging to some fishermen near Newfoundland, by twining its arms round the vessel, and which, having had two of those arms cut off by the fishermen, moved off, "ejecting a large quantity of inky fluid to cover its retreat," I desire to draw attention to an observation respecting this fluid, which I made on the occasion of a visit to the Crystal Palace Aquarium. My friend Mr. Lloyd was good enough to dislodge a cuttle from its place of concealment, and the usual inky discharge followed, as the creature shot across the tank. Mr. Lloyd states in his interesting "Hand-book to the Marine Aquarium," "that the ink (which is viscid) does not generally become diffused through the water as writing ink would be, but is suspended in the water in a kind of compact cloud till it gradually settles down, and is dispersed in flakes." Now I quite think, with Mr. Lloyd, that this being the case, it is difficult to perceive how, according to the generally received opinion, its retreat is covered by the ejected cloud. It seems to me more likely that this discharge is to divert the at-

tention of a pursuer—a dog-fish for instance—which would for the moment be startled by the sudden appearance of masses of dark colour in the water, and in the confusion the cuttle makes his escape.

Now that public aquaria are becoming so general in our great towns, it is much to be hoped that this and many other interesting problems in marine zoology may be solved.

Birmingham, Feb. 28

W. R. HUGHES

Transmission of Light in a Squall

ON the Admiralty Pier, Dover, during a "squally" gale, I remarked an occasional jerking or unsteadiness in one of the adjacent lights, say two miles off, to one of the coast-guard's men with whom I was talking at the time.

To him this was a well-known observation in squally weather. At times, he said, two lights could distinctly be seen for a second or so; frequently the shape of the light was changed, by elongation, vertically and horizontally.

The above phenomenon, if not generally known, might be worth noticing and verifying in your excellent paper.

I suppose an explanation is to be found in the different densities of the atmosphere through which a ray of light must pass in rough unsteady weather; the second image being simply the persistence of the one seen immediately before the change in position of the ray by refraction.

JAMES C. INGLIS

DR. LIVINGSTONE AND THE CAMERON EXPEDITION

IN NATURE for Feb. 26, we expressed the desire which we felt, in common with our readers, for information respecting the orders that have been sent to Zanzibar as to the disposal of Dr. Livingstone's body. We now have great pleasure in being able to announce that Lord Derby acted with the promptitude and energy which might be expected from a statesman who has always shown a warm sympathy for the cause of geography. With the concurrence of the family, his Lordship has sent a telegram ordering the body of the illustrious traveller to be sent to England, and we believe that it is to be accompanied by one or two of Livingstone's faithful negro followers.

The melancholy death of Dr. Dillon and the return of Lieut. Murphy, leaves Lieut. Cameron alone, to proceed to Ujiji, to recover the box of papers left there by Livingstone, and to prosecute further geographical exploration. Heavy unforeseen expenses obliged Lieut. Cameron, who has proved himself to be a resolute and observant explorer, to purchase stores at exorbitant rates at Unyan-yembe. The necessity for providing for the march of Murphy and Dillon to the coast, with Livingstone's body and most of his followers, is his complete justification for incurring this unauthorised expenditure, and there can be no doubt that the Geographical Society will treat its gallant emissary in a generous and liberal spirit. Cameron has suffered cruelly from fever and ophthalmia, and he is now resolutely pressing onwards in the performance of his work—the Society's work—in the face of greater difficulties than were encountered by any previous expedition. He carries with him our warmest wishes for his success, and the sympathy of every true geographer in England.

ON THE NEW RHINOCEROS AT THE ZOOLOGICAL GARDENS

A GLANCE at our list of additions to the Zoological Gardens during the last week will inform the reader that the Zoological Society has been successful in adding to its already unrivalled collection of specimens of the genus *Rhinoceros* still another species, which is exhibited for the first time in the Society's collection, and most probably in this country.

It is well known amongst naturalists that in Asia there are to be found two species of *Rhinoceros*, with a single horn developed on the top of the nose. The